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"*Disinfection*," *by* Professor S. Delepine, M.B., C.M.

Professor Delepine said, having unexpectedly been asked to speak on this subject, he would limit what he had to say to personal experience. He had had, during the last five years, frequent opportunities to study disinfectants and disinfection generally, as applied in practice.

The subject was very specially brought before them in Manchester in 1892 on an occasion when the necessity of disinfecting houses that had been occupied by patients dying of tuberculosis had to be considered. Dr. Tatham at first wished to try disinfection by chlorine and by sulphurous acid in the usual way, and he (Professor Delepine) had to test the results obtained. This was done in a very simple manner. Large drops of typical tuberculous sputa were dried on pieces of paper and placed in various parts of the rooms to be disinfected. These were placed in vessels which prevented accidental contaminations after the disinfection had been completed. They were then brought to the laboratory and their activity tested by inoculation.

It was found that sulphurous acid was perfectly harmless so far as the bacillus was concerned. The tuberculous matter resisted the effects of exposure to sulphurous acid for a very considerable length of time, and showed no marked loss of vitality or virulence. Even dry chlorine produced very little more effect in the quantities and under the conditions observed in the ordinary practice of room disinfection, as carried out at that time by the authorities.

It became evident that sulphurous acid and chlorine were practically useless under these conditions, and the author suggested to Dr. Tatham the use of a solution of some hypo-chlorite. No doubt chlorine in itself was a very powerful disinfectant, but it required the presence of moisture, and it seemed that the simplest way to generate chlorine rapidly, on the spot where it was wanted, was to employ a solution of some hypo-chlorite, which would generate chlorine and at the same time supply the water wanted.

He suggested the use of a weak solution of the cheapest of all hypo-chlorite, that was hypo-chlorite of lime. It was not necessary to use it in a pure state. Ordinary bleaching powder was quite suitable for the purpose. It wa found that when bleaching powder was mixed with water and allowed to stand

for a few hours a solution of active hypo-chlorite mixed with other salts was obtained.

If a weak solution, made with about one part of the original chloride of lime and 100 parts of water, was applied to the walls or to any paper impregnated with tuberculous matter, thorough disinfection was obtained in a few hours. Disinfection was invariably obtained in that way and, in fact, an exposure of a few minutes was generally sufficient, even with a weak solution like the one he had mentioned, when the layer of virulent sputum was not very thick.

The solution was very easily prepared. Bleaching powder could be bought by the authorities in the dry state, could be mixed with water when wanted, the fluid could then be decanted if a clear fluid was desired, and the solution was ready for use.

It was impossible to find a cheaper disinfectant or one in which it was easier to secure what Dr. Cameron wanted as a necessary element of success, viz.: a definite proportion of the active element present. The one per cent. solutions damaged papers, metal fittings, etc., to a very slight extent indeed, the mechanical action of the brush, and the dirt on the wall, being the chief sources of discolouration.

These experiments with tuberculous sputa led Professor Delepine to try the action of hypo-chlorite of lime on various other germs than the tubercle bacillus, and as was expected from the results obtained by many observers with damp chlorine, it was found that every germ tried was rapidly destroyed when brought into contact with a ten per cent, or even one per cent. solution of chloride of lime.

Dr. Woodhead had also made many experiments on disinfection, and he would be able to bear out what the author said.

Anthrax spores placed for a few minutes in such a weak solution as that he had just mentioned were killed. In the presence of results of this kind one was hardly justified in using other disinfectants, when it was possible to use chlorated lime.

There was one strong objection to the use of this solution. Hypo-chlorites when applied to walls in weak solutions generated chlorine so rapidly that the disinfectors found difficulty to remain in a room while disinfecting it. He once shut himself in a room *with every opening closed* and went through the process of washing twice the walls with a ten per cent. solution, without leaving that room. He found he could do it but was rather miserable for nearly a whole day afterwards.

It did not follow however that because the process was unpleasant to apply it should not be applied. Difficulties of that kind had been overcome before and there was no very considerable difficulty in overcoming this one. First of all he tried

to disinfect the room after all the openings had been closed, simply to try the effect of the gas evolved upon himself, but the idea that it was necessary to close the room would be in practice quite erroneous.

There was no objection to opening the doors and windows widely as the evolution of gas took place very readily under those conditions, the presence of oxygen did not interfere with the evolution of chlorine. If there was a good draught in the room in which the solution was applied, disinfection could be carried out quite comfortably.

After completing the washing the windows and doors could then be closed to retain the chlorine evolved as long as possible in the room.

There was no objection to the disinfectors using a mask with which the eyes and the mouth could be protected, the first by adequate glass coverings and the latter by a respirator. He had obtained from a well-known factory, where bleaching powder was produced, a mask which he had found to be quite efficient.

On the grounds of cheapness, efficiency, and simplicity, he thought there was nothing equal to the solution of chloride of lime. If one was frightened by the difficulties he had mentioned, the best thing after chloride of lime was formaldehyde.

He had tried lately, on an extensive scale, the lamp devised by Mr. Richard and manufactured in Belgium, and had been highly pleased to find that he could generate a very large amount of dry formic aldehyde with one of these lamps and he had been able to thoroughly kill a number of bacteria, sporing and not sporing, exposed to its action in various parts of a large experimental room.

Yet there were a few exceptions which he could not clearly understand. The exceptions were cultures of the staphylococcus pyogenes aureus, and recent moist spores of the bacillus anthracis. These had seemed to be killed at first, but after several weeks they began to grow again, showing that their growth had been only temporarily inhibited. These results he had yet to explain by making further experiments.

A thing he could not understand was that bouillon cultivations of the bacillus anthracis, with very young spores, had resisted, where dry old spores did not resist.

As he was speaking on the subject of disinfection, he might say two or three words on disinfection by steam. When the subject was considered from a purely experimental point of view, it became ridiculously simple. Steam, at a temperature of 100 to 102°c. generated extremely rapidly, and, passing in a rapid current over any object to be disinfected, would disinfect that

object in an exceedingly short time. It was sufficient to generate steam under a very low pressure (rapidly, so as to obtain a very bulky current,) to produce exactly the same results, with very simple apparatus, as could be obtained with more complicated apparatus, in which there were means of superheating the steam or of obtaining a high pressure. He had made experiments (with a sufficiently large apparatus to render the results practical), which left no doubt as to the relative value of current steam under low and high pressure, confined and superheated steam, and steam mixed with air. He had found that current steam, under low pressure, and free from air, produced complete disinfection so rapidly that nothing more was required in practice. The essential point was to obtain a rapid passage of saturated steam through the disinfector. This was accompanied by a rapid displacement of air from the apparatus, provided a large outlet was provided. A small amount of pressure was necessary to ensure the proper regulation of the temperature and current of steam, but an excessive amount of pressure was absolutely unnecessary, and made the displacement of air more difficult.

[*This discussion applies also to the papers by* Mr. Wolf Defries, Dr. J. S. Cameron, *and* Dr. H. R. Kenwood.]

Dr. Sims Woodhead (London) said that some time ago he had the opportunity of carrying out a number of experiments with various disinfectants—perchloride of mercury, iodide of mercury, and others which it was scarcely necessary to mention. He should confine what he had to say almost entirely to the "chloride of lime" and formic aldehyde, because these two were at present the special forms of disinfectants that were occupying the attention of Medical Officers of Health and those who were specially interested and experimenting in this question. After what they had heard from Dr. Cameron concerning carbolic acid, it certainly was not necessary for him to go into the use of carbolic acid as a disinfectant. As to hypo-chlorite, he had, along with his colleague, Dr. Cartwright Wood, made a large series of experiments with Hermite fluid, which was really a weak solution of hypo-chlorites obtained by electrolysing sea water. They would remember that the efficacy of the solution of that substance was found to lie almost entirely to the chlorine in various forms of combination, especially in relation to its combination

with oxygen. They then made a small series of experiments with "chloride" (hypo-chlorite) of lime or bleaching powder, and came to the conclusion that here also they had to do with exactly the same thing. Happening to mention these experiments to Prof. Delépine he learnt that the Professor was carrying on a series of experiments with bleaching powder fluid, which naturally interested him very much, and he repeated certain of the experiments that the Professor had carried out. All that it was necessary to say there was that these experiments were carried out as Professor Delépine had described them, and that in every case exactly the same results were obtained. He mentioned that, because in carrying out experiments such as these two observers often came to very different conclusions; so that when two men, or three or four men, working independently, came to practically the same result, it must be acknowledged that the method was accurate, and that the results were probably reliable. Some time ago he had an opportunity of seeing the apparatus, the working of which Dr. Kenwood had described. From what one knew of its action in solution there could be no doubt that formic aldehyde or formalin was an exceedingly powerful disinfectant. As to the method of its application in the form of vapour, he was not yet satisfied. It might be said that this was because the experiments had not been carried out in exactly the same manner as they had been carried out by other workers: but in order to remove any objections on that ground the assistance of a gentleman had been accepted, who had full knowledge of the method of applying the process, and one who had a thorough knowledge of the whole question both from the practical and chemical points of view. This gentleman, who had an interest in the process, was good enough to apply it in his (Dr. Woodhead's) own room in the Laboratories on the Embankment. This room was placed absolutely at his disposal; the conditions under which the disinfectant was to be applied were controlled by him, and the organisms to be used selected by him. A certain number of organisms were taken on diphtheria swabs, and these were placed in various parts of the room. As he did not wish to trouble them with details he would reserve them for the printed paper.

Copy of Notes from Note-book.

Formaldehyde. 17/11/96.

$2\frac{1}{2}$ litres 40 $^{0}/_{0}$ formaldehyde, 6 $^{0}/_{0}$ calcium chloride. Driven into room through fine tube at pressure of $2\frac{1}{2}$ to 3 atmospheres, for one hour. Then left for four hours. Vapour very irritating, and caused much coughing and weeping. Materials used scattered over room in the following positions, mantelshelf, waste-paper basket, chair, floor, table, and work table.

Substances used: Staphylococcus Pyogenes aureus from dip. case 798; dip. bacilli from cases 803, 818, and 819; short dip. bacilli from case 811; Staphylococcus culture from stock; potato bacillus with spores from stock; dust on cotton wool; and samples of dust left in position and collected for the purpose of making cultivations. The swabs, where nothing to the contrary is stated, were simply

placed on a bed of cotton wool, that could afterwards be burned. Some, however, were left in open test tubes, whilst others again were left in the test tube with a loose cotton wool plug in the mouth.

The following table gives the details of the experiments. The + sign denotes that the micro-organisms remained alive; the — sign, that there was no growth in the medium into which the swab or dust was introduced. Cultures made 18/11; examined 21/11.

Organisms or Material Used.	Medium in or on which it was planted after it had been treated.	Position in which Material to be acted upon was placed.	Result.
Potato Bacillus Spores.	Broth	Hearthstone	Appeared later
„ „ „	Agar	Table	—
Dust from window ...	Broth	Mantelpiece.....	+
„ „ ..	Agar	„	—
Dust from floor	Broth	„	—
„ „	Agar	„	—
Dust left on top of cupboard	Broth	Top of Cupboard	—
„ „ „	Agar	„	—
Dust left on top of lintel of door	Broth	Top of Lintel of Door..	+
„ „ „	Agar	Fairly thick layer	+
Staph. in tube.....	Broth	Table.....	—
Staph. 98	„	Floor.....	+
„	Agar	„	—
„	„	Table.....	—
„ in plugged tube.	„	Basket	+
Dip. Bac. 803	Broth	Table.....	+
„	Serum	„	+
„	Broth	„	—
„ tube plugged...	Serum	„	+
„	„	„	—
„ short 811	Broth	Mantelpiece.....	—
„	Serum	„	—
„	„	Table.....	—
„	„	„	—
„	Broth	„	—
„	„	„	—
„	Agar	„	—
„ tube plugged...	Serum	„	+
„ 818	Broth	Basket	+
„	Serum	„	+
„	„	„	+
„	Broth	„	—
„ tube plugged...	Serum	„	+
„ 819	„	Table.....	+
„	Broth	„	+
„ in tube	„	„	+
„	Serum	„	+
„ tube plugged...	„	„	+

These experiments could scarcely be looked upon as giving completely satisfactory results. It appeared that formalin vapour had very little power of penetrating to the deeper layers of the dust that was

left in position in the room. Although thin layers as removed by the swabs were sterilised, most of that left in position still contained numerous micro-organisms. This was after the action of the aldehyde had been going on for six hours, which, he took it, was a very long time. Some of the swabs had never dried, that was to say they had been prepared a little time before they were to be used. Others were used moist. Some of these swabs were undoubtedly rendered sterile, but a very considerable proportion of them at the end of the process had on them micro-organisms that were still capable of development in a very short time. He mentioned this because they had used diphtheria bacillus, which was comparatively easily killed, but in some of the swabs remained alive, although it was killed in others. The staphylococcus pyogenes aureus and, of course, the potato bacillus were much more resistant. In at least half of the *aureus* swabs the organisms remained capable of developing when placed in broth. The potato bacillus remained active. This result, however, they expected, because they did not anticipate that any disinfectant of that kind could be sufficiently powerful to kill such resistant spores as those of this organism; and it was no serious accusation to bring against any method that spores were not killed by it, unless it was one in which the disinfectant was brought into direct contact by means of throwing the substance into solution. Very few substances in the form of vapour would kill spores. A series of experiments with the tubercule bacillus had been made, but owing to the faulty conditions of the experiment no reliance could be placed on them. All he could say at present was that he certainly looked upon Prof. Delépine's method of using bleaching powder in the ratio of one per cent. as being infinitely superior to any other method at present at their command. First of all, as the Professor had pointed out, it was perhaps a little difficult to use, especially if the room was not well ventilated. The great disadvantage of formic aldehyde was that the vapours were exceedingly irritating. During the time that the experiments were being made, the effect was so marked that the operator had to retire pretty frequently, and, they who were not actually assisting, had to keep a considerable distance away, even after the room had been aired for some time. This he looked upon as a very great disadvantage indeed, because unless they could light their lamp and leave it in the room for some time, they would find it would be very difficult to induce disinfectors to have very much to do with formic aldehyde. He should say that as yet, whatever it might become, the formic aldehyde method had not been proved to be as efficient as chloride of lime solution. Its possibilities were very great because of the very great activity of this material in solution, but for the present—so far as he had been able to learn—he thought they must agree that its use as a vapour had scarcely come within the range of practical politics.

NOTE.—Since I made these remarks I have had brought under my notice several methods of applying formalin vapour, by which some of the objections mentioned above are removed or minimised. I shall take an early opportunity of repeating some of the above experiments.—G. S. W.

Dr. S. Rideal (London) said that he must differ from Dr. Cameron in his opinion that it was difficult to analyse preparations containing carbolic acid, as—although it took a little longer to ascertain the amount of carbolic acid in one of its preparations—he did not think a chemist had any difficulty in determining the percentage of pure acid in such preparations. Dr. Cameron seemed to regard creosylic acid and carbolic acid as of nearly the same value; creosylic acid being slightly weaker than carbolic, in his opinion. Creosylic acid had a much higher value than carbolic, and that was a point to remember in dealing with these preparations. When a preparation was valued on account of the pure acid it contained, the pure creosylic acid had a much greater efficiency than carbolic acid. It seemed to be a fact that these acids acted in a different way according to the material with which they were associated. Trade preparations, emulsions with soap and gelatine, and so on, had certainly a higher value than the same amount of acid of the same strength acting alone, so that the presence of the soap, etc., conferred an increased activity to the solution. Therefore, though Dr. Cameron imagined that by buying crude carbolic acid he got a very cheap article, he was mistaken, as he did not get the same efficiency out of carbolic acid as he would if it were made into what they might call a trade preparation. Dr. Cameron himself could easily manufacture an imitation of one of these trade preparations by adding some soap to the acid and forming it into an emulsion, and in that way would get a disinfectant of far greater efficiency. Other speakers had made a comparison between chlorine and formic aldehyde. Chlorine had a molecular density of 35·5, aldehyde of 15, and phenol of 47. The rates of diffusion were inversely as the square root of their densities, so that, of the three, formic aldehyde had the greatest power of diffusion. It was much higher than chlorine, and chlorine had a higher rate of diffusion than phenol. Theoretically, therefore, one saw at once that formic aldehyde would penetrate much better than chlorine. They were, of course, talking about vapours, and what he said was that the vapour of formic aldehyde would diffuse better than chlorine gas or phenol vapour; and, in fact, of all the substances they had in use for disinfection, formic aldehyde vapour had the least density, and therefore the greatest diffusibility. If, further, as the density of air was 14·4 and that of formic aldehyde was 15, their respective rates of diffusibility were as the square root of 14·4 and the square root of 15, which were practically the same number. Therefore, formic aldehyde was diffusible into air at the same rate as air into formic aldehyde. Dr. Kenwood's experience supported this theoretical conclusion, and showed that formic aldehyde diffused more rapidly than sulphurous acid, and that it would readily diffuse right through a house as shown from its easy detection by its smell, and from passing away rapidly. There was another difference between chlorine and formic aldehyde which was also important for chemical consideration. The former was an oxidising agent, and the latter a reducing agent. Chlorine acted as an indirect oxidising agent by taking away the oxygen from the water with which it was associated.

Dr. Delépine thought that hypo-chlorite had an advantage over aldehyde because oxygen was necessary. The oxygen of the air was not necessary when they disinfected with hypo-chlorite because the water in the solution supplied the oxygen required. But there was a disadvantage from this very fact, and one which he thought was borne out by many of the experiments referred to. He believed the reason of the want of success was that the chlorine, although when brought into contact with the germ, killed it, the agent only killed the germ because it oxidised the organic matter in the germ; but, on the other hand, in presence of a large amount of organic matter the chlorine might expend itself upon such organic matter before it had time to sterilise the organisms present. The disadvantages attending the handling of chlorine were absent from bromine, and this latter agent had given good results in Germany some years ago. He objected, however, to both these bodies for disinfection because they were oxidising agents. Carbolic acid, though not so efficient an agent, was a good disinfectant in its way, because it was a permanent thing. It was interesting to note that both formic aldehyde and sulphurous acid were strong reducing agents, and were therefore opposite in character to chlorine and bromine. If there was a relation between the oxidising and reducing power of these compounds and their germicidal properties, then a formic aldehyde molecule and a molecule of chlorine or of bromine should all have the same action on organisms because they were chemically equivalent. He agreed entirely with what Dr. Kenwood had said with regard to the different lamps and to the different methods of application of formic aldehyde. He had tried this substance in the same room under different conditions as against sulphurous acid. With the latter he had obtained variable results, as in some cases when the room was made as dry as possible he had obtained better results than when he had sprayed steam into the room, and this was the converse of what one would have expected. Dr. Rideal then described at some length his experiments with formaldehyde, in which he confirmed Dr. Kenwood's experiments, and showed that with a great variety of organisms both the autoclave and paraform lamps gave satisfactory results. These experiments have been published in the November number of *Public Health*. In conclusion, he suggested that as they wanted to disinfect the walls as well as to ensure aerial disinfection, he thought they should first use a spray of formalin—though not of the strength recommended by Dr. Kenwood, 2·5 per cent. A solution of formalin containing ½ per cent. was quite strong enough, as 1 in 10,000 killed most organisms. After such spraying process a paraform lamp should be used, and he believed that this combination would be found the most satisfactory in general practice.

Dr. Childs (London) hoped that they might be allowed to hear an account of the experiments which Dr. Kenwood had himself made to show the action of formaldehyde vapour on living diphtheria cultures. Between laboratory experiments and the conclusions formed with regard to the practical exercise of their teaching there

was a wide range of possibility for error. The study of the action of disinfectants on living micro-organisms,—the reputed causes of infection,—was full of difficulties, and often misleading. Not only did different species of bacteria, but even those of the same species, vary widely in their power of resisting disinfectants; so that it is not to be wondered at if different observers obtain very different results in observing the action of a given disinfectant on a given species of Bacterium. The experiments, of which he wished to hear an account had, he was sure, been conducted with the greatest care to prevent possible sources of error, and with a due amount of "control" experiments. They were of special interest in that they filled a gap between the laboratory experiments, made in test tubes, on slips of paper, etc., and the details of actual practice;—being made under conditions where the actual disinfection of a sick room infected with diphtheria was so far as possible imitated. They would like to hear also the conditions under which the similar experiments of Dr. Sims Woodhead were conducted, (the result of which differed from those of Dr. Kenwood), and especially to know the percentage of formalin vapour employed in each case.

Dr. Leslie Mackenzie (Leith) said it was not his disregard for Homer that made him give up fumigation by sulphur. It was simply because he had not the time for it. There appeared, however, from what some speakers had said to be a tendency to think too little of sulphur. Sulphur, as they knew from various investigations, was an efficacious disinfectant. What led him to give it up was the amount of time and trouble which would be required if the results were to be of any practical value. The proper carrying out of "fumigation" was very difficult, even in well constructed rooms, not to speak of broken-down cots where they could not keep the wind out, let alone keeping in the sulphur. He had been persuaded by reading Dr. Rideal's book on disinfectants to adopt "formalin" and to take the simplest method of using it by applying it in a solution to the walls of the house. He was glad to hear from Dr. Rideal that that was the simplest way; it was certainly the quickest and they could go over the ground with perfect accuracy. As to results, during the previous nine months 250 rooms had been sprayed in Leith with two per cent. "Formalin" and, so far from the spray being objected to, he had now applications for its use in even ordinary cases. People were glad to have it used instead of sulphur, which was such a nuisance. With the "Spray" he could now have four or five rooms and the furniture sprayed as well as the bedding removed, all in the course of four hours. As to the bacteriological experiments mentioned he was not competent to say one word for or against them. He was quite prepared to accept Dr. Kenwood's experiments, though with Dr. Woodhead's qualification, but until the bacteriologists were precise and agreed amongst themselves their method could not be adopted in practice. The Trillat method made it difficult to get over the ground, as did also Dr. Kenwood's. In the former the cost was 2s. 6d. per room,

whereas they could at present disinfect at 2d. per room—and that was an important consideration, especially in small communities. He did not see why they should pay 2s. 6d. when they could get the same thing done for 2d. or 3d.

Dr. Charles Porter (Stockport) said that under the valued advice and guidance of Professor Delépine a one per cent. solution of chlorinated lime had been used in Stockport for the past three years with very satisfactory results, and it was refreshing to hear that general concensus of opinion as to the value of that disinfectant. It was supplied in pint bottles at double the pharmacopœal strength, and a slight amount of permanganate of potash was added to it to colour it, and prevent it from being taken internally by mistake. They found that it did not damage the wall papers more than ordinary washing, though it slightly discoloured some of them. One could tell at once from the characteristic smell, on going into a house, whether it had been used. Professor Delépine had referred to its irritating effect on the men employed as disinfectors, but he found in Stockport that they were able to use a one in a hundred solution of chlorinated lime without any distress or discomfort. When a room had to be disinfected the windows were opened and a fire was lighted, the object being to get rid of the infected air, and then the disinfector, under the supervision of the Inspector, washed the walls down with a long-handled whitewash brush, and where this could not reach they were "hosed" with a syringe, the results being very satisfactory. The people were got back into the house in a very few hours, so that it was unnecessary to provide shelter for those whose houses were undergoing disinfection. One objection occurred to him, and that was, that if the chlorinated lime solution used as a disinfectant got into the sewers and thence to the sewage works in any quantity, it would have a rather disastrous effect on the bacterial filters. The cost of the disinfectant was about 3d.–4d. per room, which was much less than anything claimed for formic aldehyde, and they had, in addition, Dr. Woodhead's eminent authority that the lime solution was the more reliable.

Dr. J. S. Cameron (Leeds) said his object had been rather to draw the gentlemen who had spoken than to communicate anything himself. To some extent he had succeeded, but he was looking for a sign, but no sign had been given him. He wanted to know, from the chemists, whether they could count upon either a reducing or an oxygenising property in the disinfectant, and if there was a ratio between such property and the disinfecting value. He wanted also to know the ratio of germicidal value of different disinfectants. When he spoke of sulphur, used in the old familiar way, he did not suggest that it was better than "Formalin" vapour, or that it was a disinfectant of the air. He did not want it to disinfect the air. Then came the question whether "Formalin" vapour could be adapted to use in a small space, so that they could disinfect anything like a book. They had books in the Free Library which needed some

such treatment, but they could not be put into a steam or hot-air apparatus. They could, to some extent, hang them up by their backs and expose them to "Formalin." The difficulty would be to get the vapour to penetrate between the leaves. He was inclined to think, before hearing Dr. Kenwood's paper, that there might be some hope for them in that direction, but he was sorry to confess that his hopefulness in that direction had been not a little disappointed. One thing was quite clear from the discussion, and that was that a great deal remained to be achieved in regard to the use of "Formalin"—and other disinfectants. There was plenty of room for anybody, who cared to take the risk, to write a book on the subject. Dr. Rideal had written a book, but generally he rather told us what the manufacturers said about their articles than what he himself authoritatively thought. He was, perhaps, rather too modest to do that. The proper authority to undertake the work was surely the Local Government Board. He thought they might set one of their able Inspectors, who went about asking questions about loans for little hospitals, to do some useful work of this kind, which would be of service to scientific preventive medicine, and enormously help administrative Officers of Health.

Dr. H. Kenwood (London) said the main conclusions in his paper were based upon a very large amount of authority, and his own experience only served to support the evidence of a considerable number of experienced bacteriologists of repute, who had experimented independently in England, France, Germany, Italy, and America. Though he had made every effort to collect the whole of the literature on Formic Aldehyde, as a disinfectant, the fact of Dr. Woodhead's experiment came as quite a revelation to him, and he regretted that it had not been previously published, as he was under the disadvantage of having heard only the most scanty information concerning the experiments and was therefore unable to weigh its value as against the many others which had been made. He had the greatest respect for any opinion expressed by Dr. Woodhead on the subject of bacteriology, and it was difficult to suggest how it was that so many others (referred to in the bibliographical note appended to the paper), were at issue with Dr. Woodhead—for practically every worker with Trillat's new apparatus had got results which warranted him in concluding that it was one of the most powerful means of room disinfection which had ever been employed. The power of penetration of formic aldehyde was a matter which could easily be demonstrated on a black-board from considerations of its physical properties, quite apart from the bacteriological grounds on which his conclusions were based. He was ready to concede that Dr. Woodhead's opinion on most subjects was more valuable than his own, but there was one matter in which he claimed to be a superior authority, and that was in regard to the question as to whether formic aldehyde was "within the range of practical politics" or not. He had had the advantage of directing and supervising these so-called "practical politics" for some six years in his capacity of Medical

Officer of Health, and he had recently worked a good deal with formic aldehyde, and he did not agree that its employment in actual practice was at all without the pale of practical politics. In the spray methods they had no guarantee that the man employed would do the work as thoroughly as is necessary, otherwise he would advocate the use of a spray of a weak solution of formic aldehyde, perchloride of mercury, or of the hypochlorite, as recommended by Prof. Delépine, but he thought the efficiency of any system of disinfection should not be dependent on the integrity of any employée. In answer to Dr. Childs he would like to say that the reason why he did not give the details of the experiments performed was because he thought that anyone specially interested in the paper could study them in the paper itself and they would take some time to read to the meeting. But he might say that growths from the same culture were exposed under precisely similar conditions, and for the same time, in a room which was disinfected and in another which was not, and in every case they satisfied themselves that whereas the growths were active and vigorous in the room which had not been disinfected, the cultures were destroyed in the room which had been disinfected. The object of the experiments was to see if formic aldehyde, when liberated according to the methods indicated in the paper, would be found efficient in our every-day practice of room-disinfection. Of those infections against which "fumigation" is employed in general practice, the specific organism had been identified only in the case of diphtheria, puerperal fever, and erysipelas, and the diphtheria bacilli were selected in the experiments because they were convenient organisms to work with. and it was not thought that there were any material grounds for believing that the streptococci of puerperal fever and erysipelas were any more resistant.

Mr. Wolf Defries (London) remarked that it was very satisfactory to him to learn that Prof. Delépine regarded the use of at least some slight pressure as indispensable in a steam disinfector. It was also interesting to learn that he had observed differences in the disinfectant action of such disinfectors according to the rate at which steam was produced. Prof. Delépine found the most favourable results from a very rapid current of steam, and obtained as he (the speaker) understood a reduction in the disinfectant value of the apparatus when the velocity was diminished. These data appeared to have been derived from experimental laboratory apparatus or models; and it would be extremely interesting to have some figures as to the actual velocities which corresponded to the full or the reduced disinfectant value of the apparatus. These figures were the more necessary, because it had to be remembered that the sectional area of a steam disinfector was very much larger in proportion to the sectional area of the steam piping than would probably be the case in the corresponding parts of a laboratory model. Accordingly in the laboratory experiments velocities would readily be obtained which in actual practice would be quite out of the question. In regard to Dr. Kenwood's paper the speaker did not think that the

experiments described therein interfered with the balance of evidence that formic aldehyde had never yet been produced in a gaseous form capable for practical purposes of giving trustworthy disinfection. The experiments in question, so far as they had been disclosed, were open to one of the objections to which the speaker had drawn attention in his own paper, and on this ground must in any case be regarded as inconclusive; but supposing that the cultures which had been subjected to the action of the antiseptic gas had been observed for sufficiently long to warrant the conclusion that they had been disinfected, he must protest altogether against the adoption of the diphtheria bacillus as a standard for disinfection experiments. In any practical art the standard to which a process or apparatus was tested would be one of a severity at least equal to, if not slightly exceeding, the maximum which would occur in practice. It was not justifiable in so serious a matter as disinfection to deviate from this excellent custom; and in testing the disinfectant action of a process which might be called on to disinfect contagia such as those of small-pox and scarlet fever, of which the resistance was utterly unknown, it would be necessary before any satisfactory result could be obtained to use test organisms of at least a fairly high resistance, and not organisms, such as that of diphtheria, which were liable to "peg out" if one spoke crossly to them. The speaker had never suggested that any existing process for the disinfection of rooms was as satisfactory as was steam disinfection for every purpose to which it could be applied. The human error to which Dr. Kenwood had drawn attention unquestionably existed in regard to spray disinfection; but his own opinion was that this error was even more serious in the case of aërial disinfection, and he would think it more likely that an operator would fail to seal up some part of a room, so permitting the escape and dilution of the disinfectant atmosphere, than that he would in the double application of the spray omit to cover a contagious organism. An unprincipled workman could undoubtedly cause either the spray process or any process of aërial disinfection to fail, but on the one hand the speaker did not believe that the average person employed in disinfection processes was so utterly unprincipled as had been suggested, and on the other hand, if he were not, the speaker believed that it was easier for him to make sure that he covered the whole of the surface with a spray than to be certain of having achieved the far more difficult feat of entirely sealing up a room.

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